the distance of an iceberg or a hull, though he spent his days at sea, would be an ass, capable of bringing his college-training into well-merited contempt. Also, that the youth who could apply his pocket-book rule, without knowing or seeking the reason of it, would be a useful ignorant machine, creditable enough in the forecastle, passable amidships, but out of place on the bridge, and not to be desired as the product of any educational institution whatever. OLIVER J. LODGE.

University College, Liverpool.

FEW physicists will allow to pass without protest some of Prof. Perry's observations appearing in NATURE of November 19, however reluctant they may be to raise a fresh dispute on the evergreen subject to which they refer. So far as one can gather, the Professor has long since adopted one of the many ways in which the fundamental relation of dynamics may be regarded, and works himself up into a stage fury because the majority of modern physicists regard the question in a somewhat simpler and more correct way.

Prof. Perry has an admirable fondness for kindergarten methods. Let then a beginner be armed with a simple spring balance, made, say, with elastic cord, a small waggon on wheels, a number of masses, a rule, and a clock. By a few simple experiments on a level floor or table, such as one of Prof. Perry's heaven-born engineers should delight in, he can soon be made to convince himself, independently of the units in which he measures, that the rate of increase of velocity α of a body acted on by a force is roughly proportional to the force and inversely as the quantity of stuff in the body. He will thus readily grant, in a general way, that $\alpha = k\frac{F}{m}$, where k is some constant, and will be in a position to understand the absolute truth of this relation later on. If he is an English student, he will have no objection to measure distances in feet, time in seconds, the stuff that he puts into his waggon in pounds, and perhaps the pulls that he applies to his waggon in pounds weight. He will surely admit the propriety of expressing α in (feet per second) added on per second—in the unit sometimes called, on the suggestion, I think, of Prof. Lodge, the "hurry." k then becomes the number g, and the experimenter will easily convince himself that with these units it is about 32. If he is a *boná-fide* beginner, I doubt if he will ever make a set of experiments in physics which will afford him more instruction, rough though his results may be.

But with these units k is not quite constant: not *absolutely* so even if the standard pound is kept in London, Prof. Perry notwithstanding. And, incidentally, is an engineer who loads the lever of his safety-valve with a bucketful of bricks in other latitudes in just the same position as if he did the same in London? Not that I intend to imply that variations in g matter much to the professional work of men who (very properly) compound for any little discrepancy between their calculations and the ways of nature by liberal use of factors of safety, of trifling magnitudes such as 10.

We pass then to the conception of absolute measurement, the interest and value of which are not reserved exclusively for those who use the C.G.S. system, but exist in all systems. We can make $k = 1$, and write the fundamental equation in the form $\alpha = \frac{F}{m}$, most readily by adopting either of two conventions, (1) by expressing matter in pounds and forces in terms of a unit the gth part of a pound weight (the poundal); or (2) by expressing forces in the approximately constant unit the pound-weight, and matter in a new unit, also approximately constant, consisting of g pounds. Of these two alternatives Prof. Perry chooses the last. It is also the worst, for four reasons at least, viz.:

(1) That the system is needlessly complicated, through demanding the conception of two standard portions of matter; namely, the standard pound-mass whose weight is the unit of force, and the "engineer's unit" of mass or inertia (*teste* Perry) of 32·18 pounds.

(2) That the unit of force is variable or vague, unless careful reservations are made.

(3) That the unit of mass is ditto, ditto.

(4) That a majority of those who use any such system at all, already use, largely for the above reasons, the other convention, involving the idea of the poundal.

And no amount of abuse or sophistry from the non-orthodox will get over the fact that so long as we have as standards a foot, a second, and a lump of matter that we call a pound, and so long as we think of forces as we do at present, that force which, acting on that lump of matter, would give it an acceleration of one (foot per second) per second must always have a singular interest for us, entitling it to rank as a unit even with those who personally may be content to reckon the forces with which they have to deal in some other way.

But why should Prof. Perry be so dissatisfied? His students make no mistakes, and by the adoption of his shibboleth the very tender blossoms for whom he pleads will be enabled to produce luxuriant crops in the profession for which they seem so unfit. Can it be that in attempting, for instance, to become electrical as well as mechanical engineers, they find a horrible gulf between the artificial system they know and the C.G.S. units they will have to use?

And what right has the Professor to assume that all real engineers regard the question as he does? I wonder what percentage actually do so. On what platform, for instance, is Prof. Greenhill just now, to whom the idea of mass is as if it were not, absolute measurement an accursed thing, and, above all, that relic of the dark ages, Prof. Perry's 32·18 lb. unit, Anathema? If only these champions of rival heresies can be persuaded to demolish one another, there will be for the orthodox a great peace. M. J. JACKSON.

Oxford, November 24.

MAY I, as a teacher of physics, many of whose pupils enter the engineering profession, be allowed to say a word in my own defence in reply to Prof. J. Perry's scorn, expressed in an article, "The Force of One Pound," in NATURE of November 19, vol. lv. p. 49, for those who, like myself, teach my pupils the use of the poundal in dynamical calculation.

I am sure that Prof. Perry agrees with me in looking at an absolute system of measurement, whether British or metrical, as the only logical one, and where for practical purposes change of unit has to be made, there seem to me to be two courses open: (1) to make such a change in one, or all, of the fundamental units and work *ab initio* with these changed units; or (2) to work in absolute units, converting the absolute into the practical unit, by means of multiplication by a suitable factor, or, in other words, to introduce a constant of variation, different from unity, into our equations. I prefer to adopt the latter alternative where units of force have to be expressed in practical pound-weight.

Prof. Perry seems to suggest a third course, and asks us to begin with an absolute system in which the unit of force is to be one of our absolute units, the other two presumably being the ordinary foot and second. Of course, a system of theoretical dynamics could be built upon this basis, but to teach it, as we are invited to do, side by side with the C.G.S. system, would confuse the mind of any pupil unless he were an engineering student.

But there is another branch of engineering science in which exactly the same thing has to be done, namely, electrical science. Here, too, we have a system of equations which are invariably expressed in absolute C.G.S. measure, that is, in terms of units not practically in use. Here we may again either work out anew the formulæ, choosing as unit of length the quadrantal arc of the earth (10^9 cm.), and as unit of mass the 10^{-11} gm., or, as I prefer, employ the ordinary formulæ and multiply the result obtained from it by the appropriate factor when wishing to reduce the result to practical volts or amperes; or is there still a third method in which volts and amperes become the fundamental units in terms of which lengths and forces have to be measured? L. CUMMING.

Rugby, November 23.

Recent Work on the Madreporarian Skeleton.

I SHOULD like to draw attention here to a paper just published on the skeleton of Madreporaria, by Dr. von Koch, Professor of Zoology in Darmstadt, in the Gegenbaur *Festschrift*. Some time ago, in November 1895, an "abstract" was published by me in the *Proceedings* of the Royal Society, vol. lix., embodying the results of a full paper entitled, "Microscopic and Systematic Study of Madreporarian Corals." The full paper, with very numerous illustrations, will be published this month in the *Philosophical Transactions*. Prof. v. Koch does not mention this abstract in his reference-list of literature. It will be all the more interesting to those who may happen to be familiar with both papers to have set before them the more important points

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wherein Prof. v. Koch's new paper was anticipated by mine of last year, and serves to confirm my published statements.

One of the advances made in my "abstract" was the recognition of lamellar structure throughout all the various parts of the Madreporarian skeleton. And this is the main kernel of the skeletal structure as now elucidated by Prof. v. Koch. For example, he writes of the epithecal "foot-plate" in the young coral:—"This first thin little plate is afterwards more and more thickened by more or less plainly laminated deposits (*Geschichtete auflagen*) of new skeletal substance from the ectoderm" ("Das Skelett der Steinkorallen," in *Festschrift*, für Carl Gegenbaur ii., p. 253). I give the translations from Prof. v. Koch's paper as literally as possible.

In the treatment of *septal* structure the results obtained by Prof. v. Koch coincide in a very great degree with statements previously published in my "abstract," as will appear from the parallel columns below.

"Abstract," Ogilvie. (*Proceedings* Roy. Soc., 1895.)	"Das Skelett der Steinkorallen," v. Koch. (*Festschrift*, für C. Gegenbaur, 1896.)
The opacity of the "primary streak" is explained as due to "a larger amount of organic cell-material" originally present near the median plane—*i.e.* at the growing edge—of the septum. Then the passage continues: "Sections show that the fibro-crystalline structure of the septum is the same throughout its whole thickness, essentially that of a double system of thin calcareous lamellæ, either smooth or fluted, and corresponding to a deposit from opposite flaps of an invagination (*l.c.* pp. 11-12).	Regarding the structure of the septa, . . . the first-formed parts, "primary streaks" (Primärstreifen) show a more irregular structure and distinguish themselves usually by greater opacity from the "stereoplasm" or secondary part lying on either side, whose crystalline elements are more or less perpendicular to the "primary streaks." One can often plainly recognise lamination (Schichtung) in these secondary deposits (Auflagen) (*l.c.* p. 255).
Regular *curves* or *lines of growth* are evident on the septal surfaces marking the intervals between successive growth-periods. The space between two growth-curves or lines on the septal surface represents the part of the septum built up in one growth-period, and it has been called by the author a septal *growth-segment* (*l.c.* p. 12)	The fairly simple and easily recognisable structure thus described is, as a rule, rendered somewhat more complicated by the presence of *growth-streaks* (*Anwachsstreifen*) alternately darker and lighter lines, which appear in sections parallel to the surface of the septal edge, and are caused by differences in the crystallisation of the layers of thickening laid down one after the other (*l.c.* p. 256).
In certain cases "the fibro-crystalline deposit is radially symmetrical around ideal trabecular axes in the median septal plane." As examples, the markings perpendicular to the spiniform-toothed edge of the Mussa septum are quoted, and reference is made to the striæ of Galaxea septa, the ridges on the septa of Fungia, &c. (*l.c.* p. 11).	"Frequently also streaks are found at right angles to the former (they correspond to the teeth of the septal edge), and in many genera, Mussa, the Fungias, Siderastræa, and others, they can be so clearly distinct from each other that one can distinguish individual centres of crystallisation in them, and lines of separations" (*l.c.* p. 256).

Prof. v. Koch then goes on to derive the "porous" and "comb-like" varieties of septa from the simple "plate-shaped" septa, and this practically completes his contribution to the subject of septal structure. Septal varieties and their systematic importance form a large part of my paper presented to the Royal Society in July 1895, and are features shortly indicated in the published "abstract."

With regard to the question of "true" and "false" synapticulæ, Prof. v. Koch does not go beyond the distinction originally drawn by Herr Pratz. But the important fact is that his research *upholds* this distinction even while he declares it to be of small value; whereas several authors have in recent years declared it quite *untenable*. In this Prof. v. Koch's actual observations again agree with, and were anticipated by, mine; this is also the case in his statement that both kinds of synapticulæ occur alongside one another in the genus Fungia. A few farther quotations may be compared concerning other parts of the skeleton.

"Abstract," Ogilvie, 1895.	"Das Skelett der Steinkorallen," v. Koch, 1896.
"The microscopic structure of dissepiments and tabulæ is demonstrated by the author to be the same. Both are composed of a series of calcareous growth-lamellæ laid down from one surface only of the aboral body-wall of the polyp. The fibro-crystalline deposit is therefore perpendicular to the plane of contact between polyp and skeleton" (*l.c.* p. 13).	"The fine lines ("Schraffen" denoting the arrangement of the crystals) are always placed nearly perpendicular to the surface of the dissepiment; there is often in addition a very fine set of lines parallel with the surface, and in this, therefore, the dissepiments closely resemble the stereoplasm of the septum." . . . "A tabula is just the same as the sum of all dissepiments lying in one plane" (*l.c.* p. 260).
"Cases occur in both those families where the only peripheral support is afforded by the epitheca. The author is inclined to think this was the primitive form of the Madreporarian calyx, and to look upon both theca and pseudotheca as later modifications associated with retrogression of the epitheca, greater prominence and rapid growth of the septa, and very often with the processes of vegetative budding (*l.c.* p. 14).	"We may accept as a great probability that the primitive skeleton of the Madreporarian corals consisted of a lamellar shedding (Abscheidung) of lime by the ectoderm basis and epitheca, which formed a protective covering round the individual polyps" (*l.c.* p. 272).
A series of evolutionary changes are enumerated, which appeared within the group of Madreporaria during the course of geologic ages. Among others, the following occur as shortly expressed in the abstract:—"Septa became more prominent and exsert in growth; their structure became more elaborate, their surfaces fluted and richly granulated, their edges knobbed, toothed, serrated, spined" (*l.c.* p. 16).	"Provided the septa are once present in their due position, it can easily be understood how the varieties of septal structure, briefly described above, may come to originate. In a very low grade they would simply be present as small eminences of the basis and epitheca, and would then eventually grow outwards as longer processes" (*l.c.* p. 273).
"The 'Rugose' epitheca became tardy in growth, and was replaced functionally by a theca or pseudotheca" (*l.c.* p. 16).	"As soon as the wall is once present . . . the epitheca loses its significance as a supporting skeleton, and it continues to exist only as a protective covering outside, which in consequence tends to become less thick." . . . "Especially in colonies the epitheca is completely retrograde round the individual calyxes" (*l.c.* p. 274).

Naturally, Prof. v. Koch's paper of some twenty-five pages treats only a few of the questions examined and discussed in my complete paper of some 275 pages, as it will appear in the *Philosophical Transactions* of the Royal Society. Nevertheless, it is satisfactory that these few points included in Prof. v. Koch's paper should afford strong and independent evidence in favour of results arrived at by me and already published a year ago. Maria M. Ogilvie.

British Association.—Toronto Meeting, 1897.

It is possible a number of the members present at Liverpool were unable to obtain all the information they desired regarding the programme of the Toronto meeting, and others may be glad to avail themselves of an opportunity to obtain information on various subjects connected with the meeting. Kindly grant me the use of your columns to say I shall not return to Canada till probably in February, and that I shall be only too pleased to answer inquiries and give information. My address is "Canaan Lodge, Canaan Lane, Edinburgh." Alan Macdougall.

Secretary, Local Executive Committee, Toronto Meeting, 1897.
Edinburgh, December 1.

A Case of Abnormal Magnetic Attraction.

Letter, copy of which is appended, came to me this morning. May be it is of sufficient interest for your readers.

A. G. Froud.

60, Fenchurch Street E.C., December 4.

S.S. *Coronilla*, Oxelosund, November 30, 1896.

To A. G. Froud, Lieutenant R.N.R., Secretary Shipmasters' Society.

Dear Sir,—Compasses and their deviations and errors being a matter of importance, you may perhaps be interested in a case of local attraction which came under my notice here.

Whilst approaching here, Hafringe Lighthouse, bearing N.N.W. (c.m.), about six miles distance, our standard compass suddenly started swinging over an arc of sixteen points. On mentioning this to our pilot afterwards, he told me of a nineteen-fathom patch on that bearing and distance which has been found to affect compasses so, and on the latest Swedish charts the bank and its effect are noted. Going out I shall try and pass over it again. Sea was smooth and compass steady at the time. Thos. Rogers.

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